SCIENCE

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*LORD KELVIN'S ADDRESS ON THE AGE OF THE EARTH AS AN ABODE FITTED FOR LIFE.**

I.

IN the early half of the century, when the more sober modes of interpreting geological data were struggling to displace the cataclysmic extravagances of more primitive times, it is not strange that there should have arisen, as a natural outgrowth of the contest, an ultra-uniformitarianism which demanded for the evolution of the earth an immeasurable lapse of time. It is not remarkable that individual geologists here and there, reacting impatiently against the restraints of stinted time-limits imposed on traditional grounds, should have inconsiderately cast aside all time limitations. It was not unnatural that the earlier uniformitarians, not yet fully emancipated from inherited impressions regarding the endurance of rocks and the immutability of the 'everlasting hills,' should have entertained extreme notions of the slowness of geological processes and have sought compensation in excessive postulates of time. Natural as these reactions from primitive restrictions were, a reaction from them in turn was inevitable. This reaction must have ensued, in the nature of the case, whensoever geologists came seriously to consider those special phenomena which point to

*This JOURNAL, May 12, pp. 665-674, and May 19, pp. 704-711.

limitations of time. But in the earlier part of the century geological attention was absorbed in the great phenomena that testify to the vastness of the earth's history. The time for the study of limitations had not come.

Nevertheless, however inevitab le must have been the ultimate recognition of limitations, it remains to be frankly and gratefully acknowledged that the contributions of Lord Kelvin, based on physical data, have been most powerful influences in hastening and guiding the reaction against the extravagant time-postulates of some of the earlier geologists. With little doubt, these contributions have been the most potent agency of the last three decades in restraining reckless drafts on the bank of time. Geology owes immeasurable obligation to this eminent physicist for the deep interest he has taken in its problems and for the profound impulse which his masterly computations and his trenchant criticisms have given to broader and sounder modes of inquiry.

At the same time, it must be recognized that any one line of reasoning, however logically and rigorously followed, is quite sure to lead astray if it starts from limited and uncertain premises. It is an easy error to press the implications of any single phase of the complex phenomena of geology until they shall become scarcely less misleading than the looser speculations which they seek to replace. A physical deduction which postulates an excessively short geological history may as easily lead to false views as did the reckless license of earlier times. Interpretations of geological and biological phenomena made under the duress of physical deductions, unless the duress be certainly known to be imperative, may delay the final attainment of the real truth scarcely less effectually than interpretations made on independent grounds in complete negligence of the testimony of physics. It is in the last degree important that physical deductions and speculations should be regarded as positive limitations only so far as they are strictly demonstrative. Falling short of demonstration, they are worthy to be regarded as moral limitations only so far as they approach moral certainty. In so far as they are drawn from doubtful assumptions, they are as obviously to be placed in the common category of speculations as are those tentative conceptions which are confessedly but the possible foreshadowings of truth. The fascinating impressiveness of rigorous mathematical analysis, with its atmosphere of precision and elegance, should not blind us to the defects of the premises that condition the whole process. There is, perhaps, no beguilement more insidious and dangerous than an elaborate and elegant mathematical process built upon unfortified premises.

Lord Kelvin's address is permeated with an air of retrospective triumph and a tone of prophetic assurance. The former is fairly warranted to the extent that his attack was directed against the ultra wing of the uniformitarian school of the earlier decades. It might be wholesome, however, to remember that there were other camps in Israel even then. There were ultra-conservatives in chronology as well as ultra-radicals. There were ultra-catastrophists as well as ultra-uniformitarians. Lord Kelvin's contributions have as signally failed to sustain the former as they have signally succeeded in overthrowing the latter. The great body of serious geologists have moved forward neither by the right flank nor by the left, but on median lines. These lines have lain, I think, rather in the field of a qualified uniformitarianism than in the field of catastrophism. Even the doctrine of special acceleration in early times, or at other times, has made only qualified progress toward universal acceptance. The body of competent geologists to-day are probably more nearly dis-

ciples of Hutton, Playfair and Lyell than of their opponents. But such is the freedom and the diversity of belief, of attitude and of method, among geologists that *as a class* they cannot be placed either here or there in the schools, nor could they thirty-five years ago.

But we are not primarily concerned with these matters of the schools and of the past. The address presses upon our attention matters of present interest and of profound importance. Referring to his former wide-ranged estimate of the time of the consolidation of the earth, Lord Kelvin says that "we now have good reason for judging that it was more than twenty and less than forty million years ago, and probably much nearer twenty than forty (This JOURNAL, May 12, p. 271), and he gives qualified approval to Clarence King's estimate of twenty-four million years. In the course of the address he speaks of 'strict limitations,' of 'sure assumption,' of 'certain truth,' and of 'no other possible alternative;' he speaks of 'one year after freezing,' and even of 'half an hour after the solidification'; he speaks of 'a crust of primeval granite,' of a depth of 'several centimeters,' and of other details of dimension and of time and of certitude so specifically and so confidently that it must encourage, in the average reader, the impression that the history of the earth is already passing into a precise science through the good offices of physical deduction. Is this really true? Can the uninstructed layman or the young geologist safely repose confidence in these or any other chronological conclusions as determinate? Can these definite statements, bearing so much the air of irrefutable truth, be allowed to pass without challenge? What is their real nature and their true degree of certitude when tested respecting their fundamental postulates and their basal assumptions?

With admirable frankness Lord Kelvin says (This JOURNAL, May 12, p. 672): "All these reckonings of the history of underground heat, the details of which I am sure you do not wish me to put before you at present, are founded on the very sure assumption that the material of our present solid earth all round its surface was at one time a white-hot liquid." It is here candidly revealed that the most essential factor in his reasonings rests ultimately upon an *assumption*, an assumption which, to be sure, he regards as 'very sure,' but still an assumption. The alternatives to this assumption are not considered. The method of multiple working hypotheses, which is peculiarly imperative when assumptions are involved, is quite ignored. I beg leave to challenge the certitude of this assumption of a white-hot liquid earth, current as it is among geologists, alike with astronomers and physicists. Though but an understudent of physics, I venture to challenge it on the basis of physical laws and physical antecedents.

By way of preface it may be remarked that the postulate of a white-hot liquid earth does not rest on any *conclusive* geological evidence, however generally it may be entertained as a probable hypothesis. Students of the oldest known rocks are not yet agreed that these are all igneous even. But granting that they may be all either igneous or pyroclastic, there is a wide logical gap between this admission and the postulate that they were all liquid *at one time* and enveloped the whole earth. Looking quite in the opposite direction is the testimony of the complex structure and intricate combination of rocks, diverse at once in chemical, mineralogical and structural characters, which the basement complex presents. The relations of the great batholite-like masses to the enveloping foliated rocks, and of analogous combinations of intrusive aspect, imply the presence of a portion of the basement complex in the already solid state when

the remainder entered it in the liquid state. It would be a bold petrologist who would insist that it has been demonstrated that the basement complex is simply the molten envelope of the primitive earth solidified *in situ*, however much he might be disposed to entertain this view among his working hypotheses. It would be petrological hardihood to maintain that it was even a 'sure assumption.' Without denying that the basement complex may be the direct or the indirect offspring of a supposed molten state, no dogma of certitude is now admissible on geological grounds.

The hypothesis of a primitive molten earth is chiefly a deduction from the high internal temperature and from the nebular hypothesis. But it remains to be shown that the high internal temperature may not also be a sequence of an earth which grew up by meteoric accretion with sufficient slowness to remain essentially solid at all stages. An attempt has recently been made to show that a highly-heated state of the interior of the earth would have resulted from the self-compression of the mass during its accretion.† The methods of reasoning employed in this attempt were identical with those of Helmholtz relative to the heat of the sun, save that they were applied to a solid body. The computations of Mr. Moulton seem to indicate that gravitative concentration may have been an adequate cause of internal heat. In addition to this the thermal effect of molecular change and tidal kneading require recognition. Until these agencies are rigorously tested and found wanting, inferences based on the alternative hypothesis can scarcely be the ground of sure assumption. The irregular distribution of internal heat is more notably in harmony with the hypothesis of internal compressive generation than with that which makes it a residuum of a molten state whose temperature should be approximately uniform. If this irregularity be assigned to volcanic action it must be remembered that vulcanism is itself a part of the irregularity and adds to the burden of explication. Both hypotheses ultimately appeal to the same source, the gravitative descent of the earth's substance. Their differences lie in the modes of action assumed respectively, and these modes are determined by the antecedent conditions of aggregation. Has it been demonstrated that these antecedent conditions were of the one kind and not of the other?

Lord Kelvin obviously assumes a nebulous state of the earth as the controlling antecedent condition. It is not quite clear whether he adopts the complete gaseous theory of Laplace, including the earth-moon gaseous ring, or not. Apparently, however, he has not adopted the gaseous earth-moon ring, but has substituted therefor a meteoroidal ancestry for the earth, for he says (p. 706): "Considering the almost certain truth that the earth was built up of meteorites falling together, we may follow in imagination the whole process of shrinking from gaseous nebula to liquid lava and metals, and solidification of liquid from central regions outwards." A little farther on he speaks of "the gaseous nebula which at one time constituted the matter of our present earth." Without feeling quite certain that I am not in error, I interpret these sentences to mean that the matter of the earth was in a meteoroidal condition just previous to its falling together, and that it passed into the gaseous condition as a result of the heat of impact, and that from thence it shrank into the liquid and later into the solid state. If this be correct it would be interesting to learn on what grounds the older hypothesis of a nebulous ring, once regarded as a quite sure assumption, has been abandoned, and

*A Group of Hypotheses bearing on Climatic Changes. *Jour. Geol.*, Vol. V., No. 7, Oct.-Nov., 1897, p. 670.

whether the reasons for that abandonment do not bear adversely also on this modified phase of the gaseous hypothesis. The strongest objection recently urged against the Laplacean gaseous ring is the apparent inability of the feeble gravity of such a ring to overcome the high molecular velocities of its lighter constituents at the high temperatures necessary to maintain the refractory material of the earth in a gaseous condition.* In addition to this radical objection to the gaseous earth-moon ring, there is the extreme probability that, if formed, it would cool below the temperature of volatilization of rock substance before it would concentrate into a globe.

The studies to which reference has just been made seemed to show that even in the globular form it is doubtful if the earth could be volatilized without the dissociation of its water and the loss of its hydrogen by molecular projection away from the earth. The inquiry seemed even to raise a doubt whether the vapor of water, as such, or the atmospheric gases could be retained at the temperature of rock volatilization; indeed, it seemed that the oceanic and atmospheric constituents might even be in jeopardy at the temperature of white-hot lava. Without insisting that these molecular inquiries are demonstrative—for they only profess to be preliminary—they seem, at least, to justify the radical inquiry whether the hypothesis that the earth was once a gaseous nebula can be entertained with any confidence, in the light of modern molecular physics. As an abstract proposition in physics addressed to physicists would Lord Kelvin feel free to assert that the water now on the surface of the earth would be retained within its gravitative control if the earth were heated so that its rock substance was volatilized? May I be pardoned for inquiring whether Lord Kelvin has not joined the company of geologists and neglected some of the physical considerations that bear pertinently on the problem in hand?

But passing this point, and striking hands with Lord Kelvin in assuming "the almost certain truth that the earth was built up of meteorites falling together," what imperative reason is there for inferring a gaseous or even a white-hot liquid condition as a result? It goes without saying that the energy of impact of the falling meteorites would be sufficient, under assumable conditions, to give rise to the liquid condition and much more, but the *actual* condition that would be assumed by the earth would be dependent wholly on *the rate at which the meteorites fell in*. If they fell in simultaneously from assumable distances an intensely hot condition may be predicated with all the confidence of logical certitude. If they fell at as great intervals as they do to-day a low surface temperature may be predicated with equal certainty. If they fell in at some intermediate rate an intermediate thermal state of the surface must be postulated. No physical deduction can be more firm than that the temperature of the surface of the earth would be rigorously dependent on the *rate of infall* so far as the influence of infall alone is concerned. Before a white-hot condition can be regarded as a safe assumption it must be shown that the meteoroids would necessarily fall together *at a highly rapid rate;* otherwise the heat of individual impacts would be lost concurrently, as is now the case, and would not lead to general high temperature.

Now, has Lord Kelvin, or any other of our great teachers in physics or in astronomy, followed out to a final conclusion, by the rigorous processes of mathematics, the method and rate of aggregation of a multitude of meteorites into a planet, so as to be

* A Group of Hypotheses bearing on Climatic Changes. *Jour. Geol.*, Vol. V., No. 7, Oct.-Nov., 1897, pp. 658-668.

able to authoritatively instruct us as to the rapidity at which the ingathering would take place? Can the problem be solved at present with any such close approximation to precison as to determine whether a liquid or a gaseous state would or would not ensue? I assume that the most probable hypothesis relative to the distribution and movements of the meteorites is one that assumes that they consisted of a swarm or belt revolving about the sun in the general neighborhood of the present orbit of the earth; in other words, some form of meteoroidal substitute for the gaseous ring of the Laplacean hypothesis. The hypothesis may, doubtless, diverge much in detail, and, indeed, in some very important factors, but I assume that no radical departure from this can be entertained without endangering the peculiar relations of the earth to the rest of the solar system and the harmonious relations of the whole; without, in other words, jeopardizing the consanguinity of the planets. If a distribution of meteorites bearing any close resemblance to the Saturnian rings, the foster parents of the nebular hypothesis, be assumed, a definite problem is presented for determination. If the rings of Saturn, which are quite certainly formed of discrete solid matter, were to be enlarged so that they should lie outside Roche's limit, and so escape the sphere of specially intense tidal strain which will permit no aggregation, what reason is there to think that they would gather together precipitately? Does the tidal influence, which, within Roche's limit, is able to tear a satellite to pieces, cease instantly outside the limit and give place to a precipitate tendency to come clashing together? On the contrary, is it not difficult to demonstrate, by rigorous processes, even the method by which the meteorites will aggregate, much less their rate, or even to demonstrate that, apart from extraneous causes, they will fall together at all. Is not the presumption in such a case favorable to a slow rather than to a rapid aggregation? If a distribution like the meteoroidal swarms that are associated with the comets of the solar system be assumed, a definite problem is set concerning which some appeal to observation is possible. Here the observed tendency is toward dispersion rather than aggregation. In either of these assumptions, or in any other assumption, the problem involves the balance between gravitative forces, revolutionary forces and tidal forces, and the gravitative forces are not simply those between the meteorites mutually, but those between the meteorites and the central solar body and the exterior planetary bodies, a complex of no mean intricacy. Is it certain that these forces would be so related to each other as to produce a swift ingathering of the whole swarm or belt, or, on the other hand, an ingathering prolonged through a considerable period? If the latter be the case (and, in the absence of demonstration, is it unreasonable to think it quite as probable as the opposite) are there any imperative grounds for assuming that a liquid state of the earth would result? Until the rate of aggregation is worked out fully and rigorously are there any moral prohibitions, strict or otherwise, to a free interpretation of geologic and biologic evidence on its own grounds? Is not the assumption of a white-hot liquid earth still quite as much on trial as any chronological inferences of the biologist or geologist?

It, of course, remains to be seen whether the alternative hypothesis of an earth grown up slowly in a cold state, or in some state less hot than that assumed in the address, would afford any relief from the limitations of time urged upon us. At first thought it would, perhaps, seem that this alternative would but intensify the limitations. Since the argument for a short history is based on the degree to which the earth is cooled, an original cold state should but hasten the

present status. But this neglects an essential factor. The question really hinges on the proportion of *potential energy convertible into heat* which remained within the earth when full grown. There is no great difference between the alternative hypotheses so far as the amount of sensible heat at the beginning of the habitable stage is concerned. For, on the one hand, the white-hot earth must have become relatively cool on the exterior before life could begin, and, on the other, it is necessary to assume a sufficiency of internal heat coming from impact and internal compression, or other changes, to produce the igneous and crystalline phenomena which the lowest rocks present. The superficial and sub-superficial temperatures in the two cases could not, therefore, have been widely different.

So far as the temperatures of the deep interior are concerned there is only recourse to hypothesis. It is probable that there would be a notable rise of temperature toward the center of the earth in either case. In a persistently liquid earth this high central temperature would be lost through convection, but if central crystallization took place at an early stage through pressure, much of the high central heat might be retained. In a meteor-built earth, solid from the beginning, very much less convectional loss would be suffered, and the central temperature would probably correspond somewhat closely to the density. The probabilities, therefore, seem somewhat to favor a higher thermal gradient toward the center in the case of the solid meteor-built earth.

But if we turn to the consideration of potential energy, there is a notable difference between the two hypothetical earths. In the liquid earth, the material must be presumed to have arranged itself according to its specific gravity, and, therefore, to have adopted a nearly complete adjustment to gravitative demands; in other words, to have exhausted, as nearly as possible, its potential energy, *i. e.*, its 'energy of position.' On the other hand, in an earth built up by the accretion of meteorites without free readjustment there must have been initially a heterogeneous arrangement of the heavier and lighter material throughout the whole body of the earth, except only so far as the partial liquefaction and the very slow, plastic, viscous and diffusive rearrangement of the material permitted an incipient adjustment to gravitative demands. A large amount of potential energy was, therefore, restrained, for the time being, from passing into sensible thermal energy. This potential energy thus restrained is supposed to have gradually become converted into heat as local liquefaction and viscous, molecular and massive movements permitted the sinking of the heavier material and the rise of the lighter material. This slow conversion of potential energy into sensible heat is thought to give to the slow-accretion earth a very distinct superiority over the hot liquid earth when the combined sum of sensible and potential heat is considered. The theoretical difference is capable of approximate computation, and Mr. F. R. Moulton has kindly undertaken to make the computation in a simplified hypothetical case which may give some impression of the possible order of magnitude of this factor. For the purposes of the computation the earth was assumed to have been composed of 40 % of metal with a normal surface specific gravity of 7 and 60 % of rock with a normal surface specific gravity of 3. These combined would give an earth whose average specific gravity would be only 4.6. The real specific gravity (5.6) is supposed to have been obtained by compression which would amount to about 18 %. Very likely the proportion of metal is put too high and the effect of compression too low, but the purpose of the computation is only to show the theoretical

possibilities of the case. The metal is supposed to have been originally scattered uniformly through the rock material in meteoric fashion, and to have gathered thence to the center, forcing the rock material outwards so far as necessary. The heat produced, Mr. Moulton found to be sufficient to raise the temperature of the whole earth (specific heat taken at .2) more than 3,000° C. The magnitude of this result is sufficient to require the careful consideration of the potential element unless the whole hypothesis can be set aside. It is large enough to cast the gravest doubt on any conclusion based on the rate of a supposed decline of internal temperature. Complete readjustment of the interior matter, however, is not postulated under the slow-accretion hypothesis. It is only assumed that a slow readjustment has been in progress throughout the geological ages and still is in progress, and that this has changed a certain amount of potential energy into sensible heat and that this heat has contributed to the maintenance of the internal temperature of the earth.

But there are in addition, incidental factors which enter effectively into the case. The gravitative readjustment of the heterogeneous interior material is presumed to have taken place by the descent of the metallic and other heavier materials toward the center and the reciprocal ascent of lighter materials from the central region toward the surface, this being accomplished in various ways, the most declared of which has its superficial manifestation in volcanic action. Now, this process of vertical transfer, beside developing heat in proportion to the work done, as above indicated, also incidentally brings the hotter material of the interior toward the surface and thus increases the subsurficial temperature. It is a species of slow convection. This convection is in no radical sense different from that which is supposed to have taken place in the liquid earth, save that it was delayed so that the heat is available within the life era of the earth, instead of being brought to the surface and dissipated in the prezoic hot stage, when it was a barrier to the existence of life instead of an aid.

Again, in the liquid earth there were the best imaginable conditions for the intermixture of the earth constitutents and for the formation of such chemical and mineral combinations as best accorded with the high pressures of the interior. In the heterogeneous solid earth, on the other hand, such combinations were restrained and delayed and have been able to take place only slowly throughout the secular intermingling of the internal material. It, therefore, hypothetically follows that throughout geological ages, as the internal material was able slowly to readjust itself, new chemical and mineral combinations become possible. These combinations would be controlled by the high pressure in the interest of maximum density, and of hypothetically possible mineral combinations, only those would form which gave the higher density.* Thus a slow process of recrystallization in the interest of greater density would be in progress throughout the ages. This denser crystallization would set free heat. It would furthermore permit the shrinkage of the whole mass and the consequent intensification of its self-gravitation and this would in turn result in further development of heat. This large possible shrinkage meets the demands of geological phenomena at a point where the liquid earth has been felt to conspicuously fail. The losses of heat from the earth, as computed by Lord Kelvin and other authorities, and the shrinkage resulting therefrom have long been held to be quite incompetent to produce the observed inequalities. Their incompetence is now

* Professor C. R. Van Hise has worked this out elaborately in manuscript not yet published.

very generally admitted by careful students. Lord Kelvin also admits this, by implication, when he says (sec. 31, p. 706) "If the shoaling of the lava ocean up to the surface had taken place everywhere at the same time, the whole surface of the consistent solid would be the dead level of the liquid lava all around, just before its depth became zero. On this supposition there seems no possibility that our present day continents could have risen to their present heights, and that the surface of the solid in its other parts could have sunk down to their present ocean depths, during the twenty or twenty-five million years which may have passed since the *consistentior status* began or during any time however long."

In addition to this recognized quantitative deficiency, the present writer has been led to question its qualitative adaptability. The phenomena of mountain wrinkling and of plateau formation, as well as the still greater phenomena of continental platforms and abysmal basins, seem to demand a more *deep-seated* agency than that which is supplied by superficial loss of heat. This proposition demands a more explicit statement than is appropriate to this place, but it must be passed by with this mere allusion. It would seem obvious, however, that an earth of heterogeneous constitution, progressively reorganizing itself, would give larger possibilities of internal shrinkage, and that this shrinkage must be deep-seated as well as superficial. In these two particulars it holds out the hope of furnishing an adequate explanation for the deformation of the earth where the hypothesis of a liquid earth seems thus far to have failed.

But the essential question here is the possibility of sustained internal temperature. It is urged that the heterogeneous, solid-built earth is superior to the liquid earth in the following particulars: (1) It retains a notable percentage of the original potential energy of the dispersed matter, while in the liquid earth this was converted into sensible heat and lost in prezoic times; (2) it retains the conditions for a slow convection of the interior material, bringing interior heat to the surface, a function which was exhausted by the liquid earth in the freer convection of its primitive molten state; (3) it retains larger possibilities of molecular rearrangement of the matter and of the formation of new minerals of superior density, whereas the liquid earth permitted this adjustment in the prezoic stages. In short, in at least these three important particulars, the slow-built meteoric earth delayed the exercise of thermal agencies until the life era and gradually brought them into play when they were serviceable in the prolongation of the life history, whereas the liquid earth exhausted these possibilities at a time of excessive conversion of energy into heat and thus squandered its energies when they were not only of no service to the life history of the earth, but delayed its inauguration until their excesses were spent.

Let it not be supposed for a moment that I claim that the alternative hypothesis of a slow-grown earth is substantiated. It must yet pass the fiery ordeal of radical criticism at all points, but it is the logical sequence of the proposition that a swarm of meteorites revolving about the sun in independent individual orbits and having any probable form of dispersion would aggregate slowly rather than precipitately. If the astronomers and mathematicians can demonstrate that the aggregation must necessarily have been so rapid as to crowd the transformed energy of the impacts into a period much too limited to permit the radiation away of the larger part of the heat concurrently, the hypothesis will have to be set aside, and we shall be compelled to follow the deductions from the white-hot liquid earth, or find other alternatives.

But I think I do not err in assuming that mathematical computations, so far as they can approach a solution of the exceedingly complex problem, are at least quite as favorable to a slow as to a rapid aggregation. If this be so, the problem of internal temperature must be attacked on the lines of this hypothesis as well as those of the common hypothesis before any safe conclusion can be drawn from it respecting the age of the earth.

Another basis upon which the address urges the limitation of the earth's history is found in tidal friction. The limitations assigned on this basis are not, however, very restrictive. The argument is closed as follows: "Taking into account all uncertainties, whether in respect to Adams' estimate of the ratio of frictional retardation of the earth's rotary speed, or to the conditions as to the rigidity of the earth once consolidated, we may safely conclude that the earth was certainly not solid 5,000 million years ago, and was probably not solid 1,000 million years ago" (p. 670) and in a foot-note it is added: "It is probable that the date of consolidation is considerably more recent than 1,000 million years ago."

The foundations of any argument involving the relations of the moon to the earth are very infirm. In the first place, no hypothesis respecting the moon's mode of origin, or of the time in the history of the earth when it became aggregated and came into effective possession of its tidal function, can claim even a remote approach to substantiation. There is not only no-substantiated theory of the origin of the moon, but there can scarcely be said to be even a good working hypothesis, for the radical reason that the hypotheses offered will not *work*. George Darwin, who has probably studied the subject more assiduously and more profoundly than any other investigator, ancient or recent, strongly expresses the situation when he says, in his recent work on 'The Tides,' (p. 360) "The origin and earliest history of the moon must always remain highly speculative, and it seems fruitless to formulate exact theories on the subject." The annular theory of Laplace encounters in their maximum intensity the objections which arise from the application of the modern doctrine of molecular velocities. The gravitative control of an attenuated ring having the mass of the moon over its constituent material must have been exceedingly low, while the high temperature necessary to sustain the refractory material of the moon in a gaseous condition must have rendered the molecular velocities very high, so that no material except that of very high atomic weight and consequent low molecular velocity could be presumed to have been retained. But the specific gravity of the moon (3.4) seems a fatal objection to the assumption that it is composed wholly of material of very high atomic weight. Besides, it is difficult to understand how the high temperature of a ring of such attenuation could have been maintained during the time necessary for its concentration. This was less difficult when it was assumed, as formerly, that the temperature of the sun at that time was excessively high, as was also that of the earth. But modern inquiry seems decidedly opposed to the assumption of excessively high temperatures at that stage. On the contrary, it has recently been urged from different quarters that the early temperature of the sun's surface must have been much lower than at present, and this is also implied in certain statements of the address (p. 711, Sec. 43). There are also grounds for grave question as to the high temperature of the earth, as has already been indicated. Under the revised forms of the nebular hypothesis there seems no substantial reason for supposing that if the matter of the moon was once distributed in a ring about the earth, it could maintain

the gaseous condition throughout the stages of its condensation. The hypothesis therefore rests upon exceedingly doubtful premises and upon exceedingly questionable deductions from these doubtful premises.

The fission hypothesis of George Darwin has recently replaced it in favor, but the above quotation implies that even its founder does not now rest much confidence in it. The objections to the theory are several and grave. In the first place, the theory of the fission of a celestial body by high rotation, as worked out independently by Darwin and Poincaré, requires that the separated bodies should not be very greatly different in mass, *i. e.*, the smaller body should not be less than one-third the mass of the larger. But the mass of the moon is but $\frac{1}{80}$ of that of the earth, and hence it lies far outside the computed limits of applicability of the fission process.

Another difficulty lies in the effect of tidal strain itself. George Darwin, in his recent work on 'The Tides' (p. 259), assigns 11,000 miles from the center of the earth as Roche's limit. This leaves a tract of 7,000 miles above the terrestrial surface within which the earth's tidal force would be so great as to tear the moon to fragments, and, perhaps, scatter these into the form of a ring. The rings of Saturn are supposed to illustrate this form of intense tidal action. The escape of the moon, even presuming it to have been separated from the earth would, therefore, have been jeopardized by its transformation into a meteoroidal ring or swarm. If the fragments, after having been torn apart, were still sufficiently affected by a minute tide to be carried away from the earth in a slow spiral, the time occupied in passing outward beyond Roche's limit must have been protracted ; and, after their escape from it into a zone where conditions not hostile to aggregation might, perhaps, have been afforded, there must probably have been another protracted period before the aggregation of the moon would have been sufficiently advanced to give it appreciable tidal effect upon the earth. It remains, therefore, to be determined, if this hypothesis is followed, at what stage in the evolution of the moon it was sufficiently concentrated to assume effective tidal functions. This is a question also applicable to the aggregation of the moon under the Laplacean hypothesis, if it be modified so as to conform to the demands of modern scientific probability. It also applies to any hypothesis which postulates aggregation from a dispersed condition. In any case, it seems necessary to determine when the moon became full grown before it is possible to assign a positive date for the commencement of effective tidal action. It would appear that such action might be developed gradually as the material of the moon became aggregated. During such gradual assumption of the tidal function the reaction between the moon and the earth must have been of a feeble sort, and a recomputation of its amount based on a series of hypotheses which shall cover the whole ground of legitimate speculation would seem necessary before any satisfactory conclusions can be reached.

It may be urged that the computations of George Darwin following, in backward steps, by the masterly application of mathematical analysis, the stages of the earth-moon relationship give a firmer ground for conclusions. In a qualified degree this must be conceded. But it is to be remarked, in the first place, that the mathematics becomes indecisive before the origin of the moon is reached, which may signify that this is not the true line of approach to the origin of the moon, or that there is some error or defect in the assumptions. It would seem to be obvious, however, that if the tidal function was the result of a slow aggregation which began at an indetermi-

nate stage in the earth's existence the numerical results of a computation based on a full-grown moon may need radical revision.

Furthermore, the agencies which are assumed to have accelerated the rotation of the earth in its earlier history must not be neglected. If they may safely be assumed to have been competent to give the earth a rotary speed sufficient to detach from itself the matter of the moon, as is postulated in the Laplacean and the fission hypotheses in common, the same agencies, if more evenly distributed in time, might prolong the period of acceleration so that it should be coincident with that of tidal retardation and offset it in any degree that falls within the legitimate limits of assumption. We encounter here again, in another form, a deduction from the assumption of a very rapid concentration of the matter ingathered to form the earth and moon, and the consequent exhaustion of its energy in an early stage. If, however, the concentration were less rapid and less complete in the early history of the earth, as is postulated by the accretion theory, as herein entertained, acceleration might be far less advanced in the earliest stages and be greater in the later stages. Hence the retarding effects of tidal friction may have been more effectually antagonized by the shrinkage of the earth during the progress of geological history. Mr. Moulton has computed the effects of the internal change of metal and rock material, assumed in a hypothetical case on a previous page, on the speed of rotation of the earth, and found that it would accelerate the then-current rate, whatever it was, about one fifth. If, therefore, the delayed central concentration left some notable part of the acceleration to be gained during the period of geological history, and if, at the same time, a slow aggregation of the moon delayed its effectual tidal influence upon the earth and the reciprocal influence of the earth upon it, the whole history may be notably affected in the direction at once of less maximum speed and of less retardation, *i. e.*, of more near approach to uniformity.

If we turn to the geological data that bear on the question of former high rotation and subsequent retardation we find ample support for profound skepticism regarding the applicability of the tidal argument. As pointed out by Lord Kelvin, if the rotation of the earth were once notably greater than at present it should have resulted in an oblateness of the spheriod such that the equatorial regions would now be all dry land, unless the body of the earth were deformed to correspond to the slackening rotation in an almost perfect manner. But there is not the slightest evidence in the configuration of the earth of such an equatorial land tract. The equatorial belt is notably oceanic rather than otherwise. Reciprocally, there should have been, with the gradual slackening of the earth's rotation, an accumulation of the oceanic waters about the poles, but there is no geological evidence of such an accumulation in any appreciable degree. In the Arctic regions, as exemplified in Greenland, Spitzbergen and the Arctic islands of America, there are ancient shallow water deposits which lie both above and below the present oceanic level. These deposits range throughout the Paleozoic and represent in some less degree both the Mesozoic and Cenozoic eras. The nature of these shallow-water deposits is such that they cannot have been formed at great depths below the oceanic surface, so that, with the allowance of a few hundred feet, it is possible to locate the ancient horizons relative to the crust of the earth, at most or all of these periods. From these it may be inferred with great confidence that the ancient ocean surface n the Arctic regions was in numerous stages of Paleozoic, Mesozoic and Cenozoic

eras not notably different from that of today. The facts even justify the seemingly extravagant statement that at several stages in geological history, early and late, the surface of the ancient ocean did not vary a foot from that of the present, since it must have passed both above and below the present horizon repeatedly during the earth's history. Geological evidence, therefore, interpreted on its own legitimate basis, seems to lend no appreciable support to any theory that postulates a high speed of rotation for the early earth, or a low speed of rotation for the present earth, unless that hypothesis is correlated with the assumption of an almost perfect adjustability of the form of the earth to the changing rotation, in which case the argument of Lord Kelvin set forth on p. 670 stands confessedly for naught.

If we postulate a slow accretion of the earth and of the moon alike, the whole subject of the former speed of rotation of the earth and the relations of the earth to the moon take on a new aspect and invite investigation along the lines of new working hypotheses. Can it be shown that it is absolutely necessary that the aggregating meteoroids gave to the earth an exceedingly high rotation at the outset? Is not this assumption of high rotation merely an offspring of the nebular hypothesis? If the moon were aggregated slowly and came into tidal functions at a late stage, and at a distance from the earth's center quite unknown, may not all its relations to the earth have developed on much more conservative lines than those worked out by Darwin and at the same time preserve those apparently significant relations to the movements of the two bodies to which Darwin has so strongly appealed in support of his hypothesis of the history of the two bodies? In other words, without challenging the validity of Darwin's most beautiful investigation in the essentials of its method, may not a change in the premises deducible from an equally legitimate hypothesis of the original condition of the two bodies lead to results in equally satisfactory accord with the existing relations of the two bodies?

At any rate, as remarked at the outset, the time-limits assigned on tidal grounds are not very restrictive, even on the assumptions made, and when they shall be worked out on revised data in accord with the newer hypotheses they may, perhaps, even be found to favor the longevity of the earth and become one of the arguments in support of it.

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(*To be concluded.*)

PERSPECTIVE ILLUSIONS FROM THE USE OF MYOPIC GLASSES.

THE phenomena to be described occurred during the first days' use of myopic glasses, and may be grouped under the following heads:

a. There was an apparent diminution in size of moving objects—persons, animals, street cars—as compared with buildings, natural scenery, and, in general, with the elements of the background of the visual field. Here the total visual fields of the normal and of the myopic eye are equally extensive; there are the same number of projection points in each. Over this background, in the case of a myopic individual, there is distributed a relatively small number of distinct and at the same time interesting or important objects. When the near-sighted person puts on powerful glasses the number of such important and interesting distinct objects thrown upon this background is vastly increased; it is crowded with a multitude of persons, animals, trees, buildings, and the like. There are here two sets of factors whose interpretation in terms of perspective point in divergent directions. Multiplicity of objects in the visual field

means farness of the observer from the things viewed, while definiteness of detail in the individual object means nearness in point of view. In the given case there is, relatively to the number of discernible objects, an abnormal distinctness, or, relatively to their definition, an abnormally great number of objects. Adoption of the one as criterion will lead to an underestimation of size; adoption of the other will result in an overestimation of distance. The former actually obtains, and for this reason as it appears.

The dominant factor of the change in character of visual objects here is the increased distinctness of them at any given distance—the greater definiteness of line and shadow, the elaboration of detail. Such distinctness of form means in general nearness of the object to the observer. Now the near object in order to be seen as a total, a unity, must be comparatively small. The arrangement of a garden plot cannot be grasped while one walks along its paths as when viewed from a window overlooking it; the course of a river can be apprehended only when seen from some neighboring height. The same holds true of smaller as of larger groups of elements—the observer must step back in order to get the general effect—*i. e.*, to appreciate the factors as a total object. The more complex or grander the proportions of an object the farther away must be the point of view from which it can be grasped as a unity. If, then, it is so to be apprehended while yet near to the observer its parts must be small and simple. In the case in question the effect of the new glasses was thus to increase the definiteness of detail in visual objects, while these objects were still regarded as totals, a combination directly tending to produce that sense of smallness in the individual object which was actually noticed.

Another fact points in the same direction. Of curved surfaces a large radial extent can be seen distinctly by the myopic eye only when the object is a small one, and, therefore, not greatly affected by the parallactic angle. Of equally distinct objects, therefore (which in the two cases will be at different distances), the myopic subject sees less curative-extent than the normal; or, for two equally distinct objects in the myopic field of view (which are, therefore, at the same distance from the eye) greater visible extent of curvature means smallness of size. By the use of the new glasses the extent of visible curvature was thus increased, while the distinctness of the objects' details remained unaffected. This influence, therefore, coöperated with the preceding to produce the feeling of unnatural smallness in the nearer objects of vision.

b. The change in relative curvature-extent visible from the point of distinct vision appears to have been active in the production of another perspective illusion, the exaggeration of curvature in objects bounded by convex surfaces. The cheek or brow of a person, for example, appeared to bulge out unduly in the middle, and there was a constant tendency to put out the hand and test by touch the accuracy of of the sight perception. In the myopic eye the point of view of distinct vision lies so near to its object that for any given group of things the perceived curvature extent is small in comparison with that visible to the normal eye. In objects beyond the range of distinct vision, when such are not overlooked and referred to the unnoticed background, the curvature gradations are obscured and the myopic eye must depend upon other cues for its interpretation of convexity degree. It reinforces the perception by contributed curvature elements. When the finer gradations of curvature are restored to sight by the stronger glasses the contributed emphasis appears to be continued, with the result of an apparent exaggeration of curvature. I have not had

opportunity to observe if in the case of concave surfaces there is an analogous exaggeration of hollowness or depth.

c. The use of stronger glasses produced an apparent diminution in the perspective relations of objects within the visual field, which at times reached almost the vanishing point. Men and women on the street were silhouetted against the background of trees and houses, or moved like shadows over a screen. A similar reduction in perspective can be produced by piercing a bit of cardboard with a small hole, and viewing a group of objects in the middle distance through it, while the cardboard is held close to the eye. The fineness and certainty of distance perception depend greatly upon the continuity of the visual field from the feet of the observer to the object viewed, and in the last mentioned case the obscuration of this sense is due to the interruption of these conditions. In the case of myopic glasses the illusion is due, in part at least, to an underestimation of the distance of the objects, resulting from their abnormal definition as seen through the stronger glasses. In any series of uniformly spaced objects the apparent size and the visual distance between any two adjacent members decreases as their absolute distance from the eye increases. In all normal cases this decrease is correctly interpreted through the coördinated perception of increased distance. If, however, an illusion of increased nearness to the observer arises from any cause, not only do the objects themselves appear smaller, but the relative distances between them are likewise reduced, and the perspective of the field of individualized objects thereby diminished.

d. The faces of persons in the middle distance—that is, towards the farther limit of distinct vision for the character of the facial lines and expression—appeared to hang in the air near by when first caught sight of. Here the distance of the object appears to have been estimated correctly by the use of various familiar criteria, chiefly the multiplicity of objects between the observer and the person seen. When, however, the eye first rested upon the face of the person in question these cues fell into the background and the abnormal definition of the face became the dominant factor of the experience, a definition possible to the unaided myopic eye only within a much narrower range of vision; and the shock of contradiction between the felt distance of the object and its observed distinctness resulted in a dissociation of the face image from that of the rest of the body, the latter maintaining its estimated distance, the former approaching to that corresponding habitually with the observed definition. The illusion maintained itself only during a few moments while the attention was strongly centered on the face.

e. This focussing of attention upon the face had itself an abnormal element in it. The faces of persons at a distance appeared mask-like and grotesque; the eyes stared, the light and shadow fell unnaturally, the lines and expression were distorted. Subjectively this change was manifested chiefly as an alteration in the affective overtone of the object, but one which itself is derived from a change in the character of the perception. The magnitude of the visual angle which any object subtends varies with its distance from the observer. As this distance changes, the mechanism of the eye must be adjusted to keep the object in the focus of distinct vision. Up to a certain point this is possible, but beyond that limit accommodation of the eye must be replaced by approach of the point of view toward its object. The latter form of adjustment is habitual with the myopic eye as compared with the normal. In consequence the angle which the object of distinct vision subtends in the case of the myopic eye is habitually greater than in that of the normal eye. It

always sees things at a different angle—in other words, it sees a different *thing*. Suppose that for the normal eye A and the myopic eye B the ranges of distinct vision be respectively a b c and b c, and that there be viewed an object consisting of a set of plane surfaces at right angles to the line of vision of the normal eye and a second set coincident with it. The normal eye will habitually see only the set of plane surfaces at right angles to its axis of vision, and at successively greater distances from its point of view; while the myopic eye, observing the same object, will not only regard these planes at a different angle, but will see also the surfaces connecting the extremities of the first mentioned planes. In other words, the two eyes will have before them different sets of visual elements. The same principle applies in detail to all objects of distinct vision; therefore, as the point of view changes to a new focal distance from normal to myopic, or the reverse, the constituents of the visual field are altered and an accent of strangeness and unfamiliarity is given to its objects. This matter of focal distance becomes of distinct importance in photography, where the space relations of camera and object must be as nearly as possible those under which the picture will afterward be viewed; otherwise a distortion of perspective appears which materially interferes with the truth of the representation.

f. There is a final group of changes in visual perception to be considered in connection with concomitant motor adjustments. These consist, in the first place, of deflections and curvatures of right lines when viewed through the marginal areas of the glasses, which are obviously due to the non-homogeneous refractive qualities of the lens. They are identical with the distortion of vertical lines upon the sides of the visual field in a photograph the focal distance of which is short in relation to the length of these lines. The divisions of the sidewalk, the rails of the car tracks, and all lines whose direction lies at right angles to that of vision, are thus warped from the rectilinear. The same is true of house-walls and trees, and of all vertical lines at the sides of the visual field. When coming down a flight of stairs the steps curve forward at the sides, making them appear a semicircular, hollow flight.

The result of these changes is a confusion of the relations between visual perception and motor adjustment. The familiar visual cues by which the latter is habitually governed have been destroyed, and movements are awkward and mal-adjusted. It is impossible to walk down a familiar flight of steps without stumbling repeatedly. The illusionary reduction in visual size and foreshortening of perspective work disastrously here, and result in a short, mincing step which brings the foot constantly into collision with the step from which it is descending, instead of allowing it to clear for the next. There is an absolute contradiction between visual measurement and motor adjustment. The only way to secure such adjustment and reach the bottom in safety is to look quite away from the steps and to trust wholly to joint and limb perception. Thus the connections of muscular memory become the controlling cues, uncontradicted by present visual impressions, and the descent grows at once secure and rapid.

Secondly, the shortening of perspective is not uniform for all areas of the lens, but increases continuously from the margin towards the center. The effect of this appears in a curious optical illusion and a second form of mal-adjustment of motor reaction in consequence of it. The ground in front, as one walks, appears constantly to rise in a sharp curve, as if a steep hill were being mounted, and the foot is raised to meet the imaginary elevation, only to be brought down again with a shock to the original

level. It is a continual repetition of taking a step too many at the top of the stairs.

The most strongly marked characteristics of the whole experience lay in the change wrought in the affective overtone of perceptual objects in the suggestion of new touch-qualities and impulses, and the existence of abnormal emotional attitudes, but these matters lie too far afield to be considered in the present paper.

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*BIRDS AS WEED DESTROYERS.**

A MILLION weeds can spring up on a single acre. Cultivation will do much to eradicate these noxious plants, but some will always succeed in ripening a multitude of seeds to sprout the following season, so as to make tilling the soil an everlasting war against weeds. Certain garden weeds produce an incredible number of seeds. Thus a single plant of purslane may mature a hundred thousand seeds in the fall, and if unchecked would produce in the spring of the third year ten billion plants.

Probably the most efficient check upon this unbounded increase of seeds is to be found in the seed-eating birds which flock by myriads to agricultural districts to feed upon the bounty of the weed-seed harvest from early autumn until late spring. Since birds attack weeds in the most critical stage of the plant cycle, it follows that their services will be of actual practical value. The benefits are greatest in case of hoed crops, since here found the largest number of annual weeds, which, of course, are killed by frost and must depend for perpetuation solely upon their seeds. Seed-eating species of birds prevent, in a large measure, weeds of this class, such as, for instance, ragweed, chickweed, purslane, crab grass, pigweed, lamb's quarters and several weeds of the genus *Polygonum*, from seeding down the land with a rank vegetation fatal to cultivated crops. The problem of weed destruction is of such magnitnde that Mr. F. V. Coville, Botanist of the United States Department of Agriculture, in discussing weed legislation, has said, * * * "Since the total value of our principal field crops for the year 1893 was $1,760,489,273, an increase of only 1 per cent., which might easily have been brought about through the destruction of weeds, would have meant a saving to the farmers of the nation of $17,000,000 during that year alone."

The birds most actively engaged in consuming weed seed are horned larks, blackbirds, cowbirds, meadow larks, doves, quail, finches and sparrows. In a field sparrow's stomach I found 100 seeds of crab grass, in a snowflake's stomach 1,000 seeds of pigweed, and in a mourning dove's crop 7,500 seeds of *Oxalis stricta*. That the destruction of weed seed by birds is extensive enough to be of considerable benefit to the farmer is shown by Professor F. E. L. Beal, who estimated that in the State of Iowa alone a single species, the tree sparrow, consumes annually 875 tons of weed seed.

From the examination of the stomachs of some 4,000 birds it has been determined that the best weed destroyers are the goldfinches, grosbeaks and a dozen species of native sparrows.

In cities the English sparrow, assisted by several native species, does good work by feeding upon the seeds of lawn weeds, such as crab grass, pigeon grass, chickweed and the dandelion. On the lawns of the Department of Agriculture, in Washington, the birds feed upon dandelions from the middle of March until the middle of August. After the yellow petal-like corollas have disappeared, and the flower presents an elongated egg-shaped body, with a downy tuft at the upper end, the sparrow re-

*Birds as Weed Destroyers. Year-book of Department of Agriculture for 1898, pp. 221–232 inclusive.

moves several long scales of the inner involucre by a clean cut close to the receptacle, thus exposing the plumed akenes, and then seizes a mouthful of these between the plumes and 'seeds,' lopping off the plumed pappus and swallowing the 'seeds.' The mutilation of the involucre by the sparrow's beak can be seen until the flower stalk dries and falls. Fully three-fourths of the dandelions that bloomed on the Department grounds during April and May, 1898, were mutilated by birds.

The English sparrow, in spite of the services it renders in consuming weed seed, is a pest because of its despoiling buildings, and because of its extensive pillaging of fruit and grain. The native sparrows, on the contrary, have no such noxious habits, and are much more efficient as weed-seed destroyers.

The several species of goldfinches are equally beneficial. The American goldfinch confines its attacks almost entirely to the *Compositæ;* the thistle, ragweed and dandelion being its favorites. Last October I observed a flock of fifty on a New Hampshire farm. A bird would alight on a bull thistle and the pappus would float away as it feasted. Under a thistle head I found over a hundred empty akenes that had been split open on one side and had their seeds removed. These goldfinches alighted, several at a time, in a single ragweed plant and fed so busily that I could approach within a few feet of them. On another day this flock of birds fed upon the evening primrose. According to Mr. H. C. Oberholser the goldfinch also feeds upon beggar ticks (*Bidens frondosa*) and milkweed (*Asclepias syriaca*).

Dr. E. V. Wilcox has observed American goldfinches in Montana feeding in flocks of fifteen to twenty on the wild sunflower, which is a very bad pest in the West. In the same State he observed Juncos and red poll linnets eating the seeds of the Russian thistle.

The goldfinches and native sparrows are more beneficial to agriculture than a number of other species, such as the English sparrow and blackbirds, which at times injure grain and fruit, but there are, however, in the work of weed-seed destruction some fifty species of birds engaged, and the number of species of weeds which they tend to eradicate amounts to more than three score.

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THE BIOLOGY OF THE GREAT LAKES.

Science for July 1, 1898, contained a notice by Dr. H. M. Smith, of a proposed Biological Survey of Lake Erie to be carried out under the auspices of the United States Commission of Fish and Fisheries.

Unfortunately, none of the work of the season of 1898 could be entered upon until the middle of July, and it was discontinued about the first of September. Since the work outlined in the second paragraph of Dr. Smith's notice is of such a character that it must be carried on continuously, it must wait for the establishment of a permanent biological station on the lakes.

The work that could actually be undertaken was that outlined in the third paragraph of the notice. The shortness of the time (4–6 weeks) did not permit results to be reached in many of the problems under investigation; so that the results of the summer's work so far published are contained in three papers by Dr. Jennings, a brief notice of the occurrence at Put-in-Bay of *Trochosphæra solstitialis*, contained in Science, October 21, 1898, and two papers on 'The Motor Reactions of *Paramœcium*' and the 'Laws of Chemotaxis in *Paramœcium*' in the *American Journal of Physiology*, May 1, 1899. Progress was, nevertheless, made in all the other lines of work. Some of the results are now awaiting publication

and others will be ready for publication during the coming autumn.

During July and August, 1899, work will be continued at Put-in-Bay. The party will consist of the writer, as director; Professor H. B. Ward, of the University of Nebraska; Dr. H. S. Jennings, of Dartmouth College; Dr. Julia B. Snow, and Mr. R. H. Pond, besides a number of assistants. The members of the party will continue the work undertaken last summer, and referred to in Dr. Smith's notice, except that Mr. Pond, who takes the place of Mr. Pieters, will undertake an experimental investigation of of the nutrition of the larger aquatic plants.

The entire party will work at Put-in-Bay during July. During August it is intended to divide the party. Those engaged in experimental work will remain at Put-in-Bay. The writer and Professor Ward, together with a number of assistants, to act as collectors, will make a tour of the lake for the purpose of making collections, and in order to study the distribution and constitution of the plankton in the different parts of the lake.

The locality at Put-in-Bay affords a variety of conditions and is rich in aquatic fauna and flora. The occurrence of the huge infusorian *Bursaria truncatella* and of *Trochospœra* are of especial interest.

During August it will be possible to offer the facilities of the laboratory to a limited number of investigators. The United States Commission of Fish and Fisheries will furnish all apparatus, glassware and reagents and place the entire resources of the laboratory at the disposal of such investigators without charge. Those who wish to take advantage of this opportunity should communicate with the writer at Ann Arbor, Michigan, before July 1st; at Put-in-Bay, Ohio, after July 1st.

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THE INTERNATIONAL CATALOGUE OF SCIENTIFIC LITERATURE.

GEOLOGY AND GEOGRAPHY.

THE schedule of classification of writings relating to Geology and Geography which it is proposed by the International Catalogue Committee to adopt appears, on the whole, to have been well considered, though, as regards its details, it is evidently open to certain criticisms. Thus it will be noted that there is no recognition of any subdivisions of the Archæan. The matter of soils, clearly of much importance, finds no place in the list. It is hardly to be grouped under the heading of Denudation and Deposition. So, too, the matter of shore lines appears to have fairly a share in a scheme where glacial geology is ranked by itself apart. It may also be remarked that the whole field of economic geology is not suggested by any of the headings, and surely deserves recognition in any catalogue. Were this heading adopted it would naturally include a large part of the papers concerning veins and other ore deposits. As it is, these phenomena appear not to have been thought of.

Under the heading of Geography is a schedule of classification on a topographic basis, which is probably intended to serve also for the distribution of a portion at least of the works on geology, though this is not clearly stated. As a whole, the topographical classification which has been adopted commends itself to the reader. In places, however, the meaning is not clear, as in '*K* Arctic: Greenland and the area north of the Arctic Circle, or all the coasts of Continental America, Asia and Europe, *whichever is farther north*' (the italics are ours). It is possible, by systematic exegesis, to arrive at some conception of what the writer meant, but at first sight it seems to imply a variable *northness* of these several areas. It may also be noted that the category denoted by *ea.*, viz., Asiatic Russia, is much too

large for convenience. In time a great literature will, doubtless, have to be referred to this division. The realm could be subdivided, perhaps, on the base of its drainage.

Under *gb.*, *gd.*, *gd.* and *ge.* the division is troublesome. First, we have Canada as a whole, then the Canadian Dominion west, including Yukon and British Columbia, Mackenzie, Athabaska, Alberta, Saskatchewan and Assiniboia; but *gd.*, the Canadian Dominion east, includes only Newfoundland. In this specification Labrador and the neighboring districts seem to be left out. To add to the confusion comes *ge.*, which takes in the Laurentian Lakes, with no statement as to the limits of the territory included in the category. Following down, we find, after the United States as a whole, a division which includes the northeastern field, *i. e.*, all the States east of the Mississippi down in general to the Ohio and the Potomac, but omitting in the list Maryland and Delaware. The southeastern United States east of the Mississippi does not include a list of States. It may be intended to contain those last mentioned, but under the circumstances the names should be specified.

The subject classification under Geography is, as will be observed, much more detailed than the like grouping under Geology. It appears tolerably complete, but there again the matter of soils and of shores is omitted, though such matters as rocks, minerals and mines, which are less fitly to be regarded as geographic, find a place. It may also be noted that, while under Geology there is a 'seismic' division (including elevation and depression and mountain building), the matter of earthquakes is only mentioned in the geographic classification. We may fairly wonder whether this suggested difference in treatment was actually designed. In the geologic scheme volcanoes are included. They can come in again under the head of volcanic phenomena under Geography. Again we wonder whether this arrangement is by chance or design.

If it is intended by this classification to demark the fields of geology and geography it is clearly open to objection from many points of view. Thus such matters as dunes, coral reefs, minerals, mines, etc., which find mention only under Geography, are, by common understanding, regarded as subjects for treatment under the head of Geology.

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PHYSIOLOGY.

THE Editor of SCIENCE has kindly asked me to comment on the physiological part of the Catalogue of Scientific Literature, prepared by the Royal Society. I should like to call attention to a few points.

1. It seems to me that the space given to *comparative physiology* is not sufficient. Physiology is undergoing the same change that has taken place in morphology. The latter science was originally confined to man and a few of the higher vertebrates, but at present scientific morphology is identical with comparative morphology. The same change is taking place in physiology. It is true that the text-books of physiology have as yet taken no notice of this change, but a catalogue of 'scientific literature' cannot afford to ignore the development of physiology. The catalogue must take into consideration the fact *that the field of comparative physiology is much wider than that of human physiology*, and that, therefore, more space and a more prominent position is necessary for comparative physiology than is allotted to it in the provisional schedule.

2. It seems to me that *physical chemistry* has not received the consideration it deserves. It is hard to tell, for instance, in which part of the catalogue the effects of ions should be mentioned. There is a sub-

division on isotony and other osmotic phenomena of the cell, and there is another subdivision in physiological chemistry on 'semi-permeability and physiological properties of colloids,' but I am at a loss to find where experiments on the osmotic properties of muscles or connective tissues, etc., could be properly catalogued. It seems to me that fuller provision should be made for the whole realm of the application of physical chemistry to physiology.

3. It seems to me, further, that provision should be made for the facts of physiological morphology. By physiological morphology I mean the energetics of the phenomena of organization. Physiology has thus far chiefly been a study of the phenomena of irritability. But there can be no doubt that phenomena of growth, irritability and metabolism are so thoroughly interwoven that neither metabolism nor irritability can be fully understood without taking into consideration the phenomena of growth. For instance, only the active muscle is able to undergo hypertrophia. The resting muscle atrophies. It is evident that contractility and growth are in some way connected. In plants the heliotropic and other curvatures are connected with the phenomena of growth. It is even possible that our inability to explain contractility is due to the fact that we have not yet taken into consideration the phenomena of growth. Furthermore, I do not quite see where in the present catalogue such experiments on physiological morphology as those on heteromorphosis (the experimental substitution of one organ for another) could be mentioned. Physiological morphology includes also the physiological analysis of heredity. The field of physiological morphology is wider and certainly more fundamental than the present physiology of nerves and muscles.

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SCIENTIFIC BOOKS.

Talks to Teachers on Psychology, and to Students on some of Life's Ideals. By WILLIAM JAMES. New York, Henry Holt & Co. 1899.

In his first chapter Professor James discusses the relation of psychology to the teaching art. We have so many statements from non-psychologists concerning what psychology may do for teaching that it is pleasant to hear what a psychologist himself has to say on the subject. In the first place, it is pointed out that sciences do not directly generate arts. The study of logic does not make a thinker, nor that of grammar a correct speaker; so the study, even the mastery, of psychology does not insure success in teaching. A science and its corresponding art can be brought together only by means of a mediator; that is, a mind full of tact and invention for the application of the rules of the science to the practice of the art. Given a skilful mediator, psychology can be of the greatest aid to teaching. This is especially true in this country, where the system is so elastic that it becomes a vast laboratory for educational experiment. To this advantage we have the concomitant circumstance of a body of psychologists anxious to instruct another body of teachers eager to learn.

Incidentally, in this chapter, Professor James attempts to allay the pangs of bad conscience in those teachers who have been made to feel that they must contribute to child psychology or be unworthy their calling. He heartily agrees with Professor Münsterberg that the psychologist's attitude toward mind must be abstract and analytical, whereas the teacher's should be concrete and ethical. Haunted by Emerson's lines—

> "When duty whispers lo, thou must,
> The youth replies, I can,"

the conscientious teacher is pained that she does not. But Professor James eases this pain by intimating gently that obligation is obviated by inability.

The second chapter contains an abridgement of Professor James's well-known description of the Stream of Consciousness, while the third and fourth chapters are devoted to conduct as the outcome of education.

Chapters five, six and seven show the nature and need of spontaneous and acquired reactions. This discussion is new, forceful and illuminating. Not all of these things can be said of the succeeding chapter on the laws of habit. This is taken almost bodily from the author's 'Psychology.' That it is brilliant and sound will be attested by many. Yet what shall we say of the man who *can* produce new books, but who simply copies his old ones verbatim in the most important parts? Professor Patten, in his 'Development of English Thought,' declares that geniuses are always lazy. Professor James can bear this double imputation, yet one can hardly excuse him when he says he needs to offer no apology for copying his own books. The apology is needless only because it is useless. An author should treat himself as well as he treats other authors. He would not incorporate their matter without transforming it by the force of his own thinking; no more should he repeat himself without subjecting his older thought to the transforming influence of a new point of view. Who wants to buy the same book twice?

The chapters on Interest and Attention are among the best and most typical in the book. The treatment is eminently popular and general, yet none the less helpful on that account. If it is much less rigid than that of Dr. Dewey, it is perhaps as useful to the ordinary teacher. The difference is that which exists between a diagram and a demonstration; the one is æsthetic, the other intellectual.

Apperception is described at some length in chapter fourteen, the discussion making no pretension to scientific exactness. Indeed, Professor James has always given the topic a step-motherly treatment, viewing the word *apperception* as a blanket term in psychology, and following the older traditional division into sensation, perception, memory, etc. Yet even from the standpoint of psychology itself, the researches of Wundt and others have shown that there are distinct advantages in treating apperception as an elemental process in psychic life; when we come to education the advantages of this procedure are great and unquestionable. It is to be hoped that Professor James will some day give his mind to a thoroughgoing scientific exposition of the subject. If one may be permitted to cut out work for his neighbor, one may perhaps suggest to Professor James that a monograph upon apperception in its educative bearings would be gratefully received by American teachers.

Of the significance and value of this volume as a contribution to the cause of education there can be no question. Like everything that Professor James writes, it is at once lucid and interesting. If the treatment is popular and general, it is, at all events, founded on scientific insight, and, so far as it goes, may be confidently trusted as sound. If it ridicules 'brass instrument' study of children, it yet tends to awaken sympathy with childhood. If it disappoints the seeker after 'scientific' study of education, it, at least, satisfies the heart of the earnest teacher.

Finally, this book is to be welcomed because it shows that in educational theory, as in treatises upon subject-matter, the writing of books is passing from the hands of professional bookmakers into those of the real leaders of thought. In this fact we find the brightest hope of our educational progress.

CHARLES DEGARMO.
CORNELL UNIVERSITY.

Wetterprognosen und Wetterberichte des XV. und XVI. Jahrhunderts. No. 12, Neudrucke von Schriften und Karten über Meteorologie und Erdmagnetismus herausgegeben von PROFESSOR DR. G. HELLMANN. Berlin, A. Asher & Co. 1899.

In this volume, which is the latest and largest of the series, Dr. Hellmann explains the origin and growth of weather predictions in almanacs, etc., and the practice in the different countries of describing remarkable meteorological phenomena, illustrating both subjects by *facsimile* reproductions of printed documents of the fifteenth and sixteenth centuries. As Dr. Hellmann remarks, the art of foretelling the weather has always been the object of meteorological research, but it has been practiced in various ways according to the theoretical knowledge that existed of the occurrences in the atmosphere. Among the Greeks, at the time of Meton, public placards announced the past and expected weather. Later, astrology controlled

the predictions in the almanacs, which were first printed in Latin and afterwards in the language of the country where they appeared. Such an almanac, the *Bauern-Kalender*, or peasants' calendar, having symbols to represent the predicted weather, is still published in the Austrian Tyrol. The custom of writing accounts of extraordinary meteorological events is very old, and, after the invention of printing, these reports, in pamphlet form or on single sheets, were widely distributed throughout Europe. As they were intended for the people, few have been preserved in libraries, but some of these are here reproduced.

The volume contains 33 pages of historical and critical introduction and 26 *facsimiles* of German, French, English, Italian, Spanish, Danish and Dutch tracts, most of them curiously illustrated. Probably to no other person than Dr. Hellmann would so many rare works in all parts of Europe be accessible, and his scholarly preface greatly aids the comprehension of these interesting specimens of ancient weather lore. One or two copies of the volume may be obtained for the publisher's price, viz., 20 Marks, or $5, from the undersigned, at Hyde Park, Mass.

A. L. ROTCH.

BOOKS RECEIVED.

Proceedings of the Fourth International Congress of Zoology, Cambridge, 22–27 August, 1898. London, C. J. Clay & Sons. 1899. Pp. xiv + 422 and 15 plates. 15s. net.

Cinématique et mécanismes potentiel et mécanique des fluides. H. POINCARÉ. Paris, Carré et C. Naud. 1896. Pp. 385.

Alaska and the Klondike. ANGELO HEILPRIN. New York, D. Appleton & Co. 1899. Pp. x + 312.

Leitfaden der Kartenentwurfslehre. KARL ZÖPPRITZ. Leipzig, Teubner. 1899. Pp. x + 178. Mark 4.80.

Der Gang des Menschen. II part. OTTO FISCHER. Leipzig, Teubner. Pp. 130 and 12 plates. Mark 8.

Elektrische Untersuchungen. W. G. HANKEL. Abhandlung der mathematisch-physischen Classe der königlichen sächsischen Gesellschaft der Wissenschaften. Leipzig, Teubner. 1899. Vol. 24. Pp. 471–97 and 2 plates. Mark 2.

Practical Physiology. DR. BURGH BIRCH. Philadelphia, Blakiston's Son & Co. 1899. Pp. x + 273. $1.75.

The Steam Engine and Gas and Oil Engines. JOHN PERRY. New York and London, The Macmillan Company. 1899. Pp. viii + 646.

Geological Results, based on Material from New Britain, New Guinea, Loyalty Islands and elsewhere, collected during the years 1895–7. Cambridge, The University Press. 1899. Pp. 356 and 5 plates.

SCIENTIFIC JOURNALS AND ARTICLES.

The Journal of Geology, April–May, 1899.—H. F. Reid, 'The Variations of Glaciers,' pp 217–225. Professor Reid presents the fourth of his summaries of observations on the advance and retreat of glaciers in different parts of the world. While recession is the rule, there are some instances of advance, and some evidence has been gathered of recurrent cycles of maxima and minima. In the case of two Swiss glaciers the periods proved, respectively, 44 and 51 years.

G. C. Curtis and J. B. Woodworth, Nantucket, 'A Morainal Island,' pp. 226–236. The former author describes a recently constructed model of Nantucket, and the latter its geology.

Mark S. W. Jefferson, 'Beach Cusps,' pp. 237–246. The small cusps along beaches are explained by the action of retreating high waves, whose waters breach the strip of seaweed that is usually present just above the line of ordinary waves, and that binds the shingle together. Between the breaches the cusps gather at intervals of ten to forty feet.

Walter D. Wilcox, 'A Certain Type of Lake Formation in the Canadian Rockies,' pp. 247–260. Interesting data are given regarding the glacial phenomena of the Canadian Rockies, and particularly regarding Lake Louise. A means of estimating the time since the retreat of the great ice sheet is suggested, but for lack of the necessary apparatus it has not been carried out.

J. P. Goode, 'The Piracy of the Yellowstone,' pp. 261–271. Recent changes in the drainage of Yellowstone Lake are described and explained. The Yellowstone River, as at present known, appears to be of development in late geological time.

C. E. Monroe and E. E. Teller, 'The Fauna of the Devonian at Milwaukee, Wis.,' pp. 272–

283. Recent excavations for the the Milwaukee water works have made available a large quantity of loose rock, which proves to be rich in Devonian fossils. These have been identified and tabulated by the authors.

H. S. Washington, 'The Petrographical Province of Essex Co., Mass.,' pp. 284–294. This paper on the basic dikes concludes the series.

Under 'Reviews' an excellent summary by T. A. Jagger is given of the recent valuable experiments of Morosewicz in the artificial production of rocks and minerals.

American Chemical Journal, June, 1899.—'The Valuation of Saccharin,' by E. Emmet Reid. By boiling for two hours with a hydrochloric acid solution of the proper strength and then distilling with alkali, the ammonia can be collected in a standard acid solution and readily determined. It was shown that para sulphamine benzoic acid was not acted upon under similar conditions. This, therefore, appears to be a quick, accurate method for determining the amount of the sweetening substance in the commercial saccharine. 'Some Derivations of Camphoroxime,' by G. B. Frankforter and A. D. Mayo. 'Camphoroxime Derivatives,' by G. B. Frankforter and P. M. Glasoe. 'The Laboratory Production of Asphalts from Animal and Vegetable Materials,' by W. C. Day. The author has obtained substances similar to the natural asphalts by distilling animal and vegetable matter, both separately and mixed. 'The Composition of Nitrogen Iodide and the Action of Iodine on the Fatty Amines,' by J. F. Norris and A. I. Franklin. The evidence points to the fact that the compound formed by the action of iodine on ammonia is not a direct addition-product, nor do the fatty amines form such compounds. 'On the Action of Sodic Ethylate on Tribromdinitro Benzol,' by C. L. Jackson and W. Koch. 'The Action of Sulphocarbanilide on certain Acid Anhydrides,' by F. L. Dunlap. 'The Action of Ammonia and Amines on Chlorides of Silicon,' by F. Lengfeld. The chlorine is replaced by the ammonia and amine residues, forming amides of silicon.

J. E. G.

APPLETON'S *Popular Science Monthly* for July has as a frontispiece an excellent portrait of Professor W. K. Brooks, and the number contains a sketch of his life and scientific work. The number contains articles by President D. S. Jordan, describing the succession of fishes inhabiting a brook; by Professor W. K. Brooks, entitled 'Thoughts about Universities;' by Professor Edward Renouf, on 'Acetylene,' and by Dr. C. C. Abbott, on 'The Antiquity of Man in North America.'

WE regret that the *Index Medicus* has been discontinued. It is unfortunate that the efforts for its continuation have not been successful, but the mass of medical literature has become so great, and, it must be added, in most cases so unimportant, that an index would require some form of public support.

SOCIETIES AND ACADEMIES.

THE NEW YORK ACADEMY OF SCIENCES—SECTION OF BIOLOGY.

THE Section met on May 8th, Professor F. S. Lee presiding. The following program was then offered:

1. W. A. Rankin: 'The Crustacea of the Bermuda Islands, with Notes on the Collection made by the New York University Expeditions to the Bermudas in 1897 and 1898.'

2. H. F. Osborn: 'Upon the Structure of the Mule-footed Hog of Texas.'

'Upon the Structure of *Tylosaurus dyspelor*, including the Cartilaginous Sternum.'

Professor Rankin's paper gives a list of 61 recorded species of Crustacea from the Bermuda Islands. During the summers of 1897 and 1898 a party from the New York University spent a few weeks investigating the fauna of the islands, and the Crustacean collections were studied by the author.

Of the total number of species 43 were found by the expedition, and notes on their distribution are given. Eight of these species are new to the Bermudas, and two, *Nika bermudensis* and *Alpheus lancirostris*, are new species described and figured in this paper. The genus *Nika* is now for the first time recorded from the West Atlantic region.

The physical conditions of the islands are touched on, and the Crustacea are shown to be in the main similar to those found in the West

Indies and the adjacent coasts of America, though 18 have a more or less extended range over both hemispheres.

Professor Osborn reported upon the anatomy of the feet of a specimen of the well-known 'mule-footed hog' of Texas, recently presented to the Zoological Museum of Columbia, by Dr. Wickes Washburn. Externally the feet present the appearance of complete fusion of the third and forth toes. Internally, however, considerable differences are observed. In the pes the third and fourth metapodials and the first phalanges are entirely separated and normal, the second pair of phalanges are closely united, and the terminal phalanx is also closely united, so it has the appearance of a single element. The fusion is less advanced in the manus; here the metapodials, first and second phalanges are separate, one of the second phalanges being abnormally hypertrophied and a supernumerary element being inserted beneath it. The terminal phalanges are very firmly united into a single element, which holds the bones above it together. Some discussion followed, during the course of which Professor Bristol stated that a large number of experiments were being carried on at a Western ranch to ascertain the effects of breeding upon this peculiar variety. Professor Osborn remarked that this anomaly presented an interesting case of the persistence of a character which must have originated as a sport.

Professor Osborn's second paper included a description of the remarkably complete skeleton of a Mosasaur, recently mounted in the American Museum of Natural History. The skeleton was procured two years ago from the famous Smoky Hill Cretaceous beds of Kansas, through Mr. Bourne, and has been worked out with the greatest care. It is practically complete as far back as the 78th caudal, and the bones are approximately in position, including the fore and hind paddle and, what is more remarkable, the almost complete cartilaginous sternum, sternal ribs, epicoracoids. The species represents the largest type of American Mosasaur, *Tylosaurus dyspelor* Cope. As illustrated by numerous photographs and drawings, the specimen throws a flood of new light upon the structure of the Mosasaurs. The principal characters are the following: 7 cervicals, 10 dorsals connected with the sternum by cartilaginous ribs, 12 dorsals with floating ribs, one sacral and 72 caudals (out of a total number of 86), coracoids connected by broad epicoracoids having a transverse diameter of 22 cm. The sternum is triangular in shape, tapering posteriorly and having the general form of that in *Trachydosaurus;* there is no evidence of an episternum, the shoulder girdle in general being more degenerate than Platecarpus, in which an episternum has been observed. The fore paddles are smaller than the hind ones and include two co-ossified carpals. The fifth digit is somewhat enlarged and set well apart from the others. The hind paddle is slightly larger and very completely preserved. The tail is remarkable in presenting an upward curvature in the mid-region, which probably supported a prominent caudal fin, but it is not angulated as in *Ichthyosaurus.* The skull shows the presence of epipterygoids. The total length of the skeleton as preserved is a little over 270 feet; the estimated total length of the animal is 30 feet. In mounting, a single large panel has been used, the animal lying upon its ventral surface, with the paddles outstretched, the sides of the back bone curved in a graceful manner, exactly as originally imbedded in the matrix.

FRANCIS E. LLOYD,
Secretary.

THE NEW YORK SECTION OF THE AMERICAN CHEMICAL SOCIETY.

THE regular meeting of the New York Section of the American Chemical Society was held on Friday evening, the 9th inst., at the Chemists' Club, 108 West Fifty-fifth street, Dr. William McMurtrie presiding. The following papers were read: 'Apparatus for testing the Density of Cements,' by Morris Loeb, Ph.D.; 'The Determination of Sulphur in Bitumens,' by S. F. and H. E. Peckham.

The apparatus described by Dr. Loeb is a modification of the well-known method for determination of the density of powders by displacement of liquid contained in a flask, but by the system of calibration adopted and the use of a specially graduated burette the volume of liquid displaced is obtained by difference

between the amount added from the burette and an arbitrary volume contained between two marks on the neck of the flask.

Drawing out the liquid to the zero mark by a pipette enables one to make another and several successive determinations without cleaning out the apparatus until the flask is actually almost filled with the powdered cement, so that three or four determinations may be made in about ten minutes.

Messrs. Peckham's paper recommended the deflagration method for determining sulphur in bitumens, using about two parts bitumen to thirty parts of mixed sodium carbonate and potassium nitrate. Some discussion followed as to the possible loss of volatile sulphur compounds—mercaptans, mercaptids and sulpho ethers—but the amounts of these forms of sulphur were conceded to be extremely small and probably without appreciable effect on the behavior of an asphalt.

A report by the Committee on Patent Legislation was read by Major C. C. Parsons, with the recommendation that it should be brought before the members of the Society at large.

A report by Durand Woodman, Secretary and Treasurer, stated that nine regular and two special meetings had been held, at which thirty-seven papers were read. The average attendance at these meetings was sixty-five.

The expenses of the Section had been $1.19 per member for the year. The membership numbers about 305.

The election of officers for the ensuing year resulted as follows: Chairman, C. F. McKenna; Secretary-Treasurer, Durand Woodman; Executive Committee, William McMurtrie, E. G. Love, G. C. Stone; delegates to the Scientific Alliance, E. E. Smith, M. T. Bogert.

A SPECIAL meeting of the Society was held on Saturday, May 27th, at 8:45 p. m., in the Assembly Room of the Chemists' Club.

Announcement was made by Dr. C. A. Doremus of the preliminary program of the Fourth International Congress of Applied Chemistry, to be held at Paris next year. The meetings will be held in the halls and amphitheatre of the new Sorbonne, and every important branch of applied chemistry will be covered.

The feature of the evening was a paper by Dr. H. W. Wiley on 'The Chemistry of Nitrification,' fully illustrated by lantern slides.

DURAND WOODMAN,
Secretary.

THE WASHINGTON BOTANICAL CLUB.

REGULAR meetings of the Club were held on May 3 and May 30, 1899. At the former the members participated in a symposium on the topic 'The Origin of Insular Floras.' Discussion was opened by Professor E. L. Greene, Dr. F. H. Knowlton and Mr. O. F. Cook. In the short notes which preceded attention was called to the discovery of *Asplenium ebenoides* in the District of Columbia, and proof sheets of Professor Bailey's 'New Encyclopædia of Horticulture' were exhibited.

The meeting of May 30th was devoted to a discussion of the more salient features of the District flora, several specimens being exhibited. The Club held a most enjoyable excursion on Decoration Day, to which other botanists were freely invited, visiting Plummer's Island, in the Potomac, and the neighboring Virginia shore.

CHARLES LOUIS POLLARD,
Secretary.

PROFESSOR DEWAR ON LIQUID HYDROGEN.

THE second lecture in connection with the Royal Institution's centenary was given by Professor Dewar on June 7th. Professor Dewar said, according to the report in the London *Times*, that he did not intend to take any long flight into the great work of the Royal Institution in the past, since that had already been done by his colleague. His object was rather to introduce his audience to a new instrument of research—that was to say, to liquid hydrogen. This he exhibited boiling gently in a vacuum tube immersed in liquid air, the access of heat being, by this precaution, greatly impeded. They would notice it was a transparent liquid, in which there appeared a whitish deposit. This consisted of solid air, and it was impossible to avoid its presence, because immediately the cotton-wool plug was removed from a vessel of liquid hydrogen the air of the atmosphere came under the influence of so low a temperature as to be at once frozen

solid. To prove that the liquid he was manipulating with such freedom was really liquid hydrogen Professor Dewar put a light to a small quantity, a brilliant burst of flame being the prompt result. Of its exceedingly small density he gave an idea by showing that a light material like cork would not float on its surface, but sank to the bottom as if it were lead. The lowness of its temperature he illustrated by a number of experiments. Thus a solid body immersed in it for a short time was shown to become so cold that the air round it was liquefied and ran off in drops, while when a tube containing liquid air was plunged into it the air immediately became solid. On this tube being lifted out again a double effect was seen, for the melting of the solid within it yielded liquid air, which was also formed by condensation on its outside surface. An empty vessel placed for a short time in the cold atmosphere just above this liquid, filled with solid air in the form of snow, soon melted into liquid. Oxygen in a sealed tube when lowered into it quickly became solid, and when lifted out it could be seen, as heat was absorbed, to assume first the liquid and then the gaseous form. A sponge of porous material, soaked in liquid hydrogen and brought into a magnetic field, apparently behaved as if it were magnetic. That, however was due to the condensation of the oxygen of the air, which, of course, was magnetic, and, though an observer might in this way be easily deceived into thinking hydrogen magnetic, Professor Dewar said he was satisfied that it was nearly neutral or diamagnetic.

Speaking of the real temperature of this liquid, he said it had taken him nearly a year to come to a definite conclusion on that point because he could not get any two thermometers to agree. Pure platinum resistance thermometers gave 35° absolute (or 238° below zero Centigrade), one of the platinum-rhodium alloy 27°, while hydrogen itself in a gas thermometer gave 21°, a reading nearly identical with one obtained with a German-silver electrical thermometer. The last part of the lecture was devoted to the extraordinarily low vacua obtainable by the use of liquid hydrogen. Thus, by immersing one end of a closed tube in it for a short time and then sealing it off in the middle, a vacuum was formed in the upper part which was substantially perfect, as was shown by the fact that the electrical charge could not be made to pass. In conclusion, Professor Dewar, after exhibiting several other beautiful experiments, including one to illustrate the rapidity with which gases were discharged into a vacuum, claimed that the liquefaction of hydrogen was a triumph for theory not less than for practice.

Lord Kelvin, in moving a vote of thanks to Professor Dewar for his brilliant, beautiful and splendidly interesting lecture, said that if those present wished to measure the importance of the occasion, let them think what Count Rumford, or Davy, or Faraday would have thought, could they have been present. They could not have hoped for their scientific dreams and prophecies to be so splendidly verified within the century. The end of experiment in research at low temperatures had by no means been reached, and perhaps in a few years substances yet unknown and more refractory than hydrogen would have been found which would bring the experimenter to within five degrees of the absolute zero.

AUTOMATIC SHIP-PROPULSION.

AUTOMATIC ship-propulsion is once more proposed, this time by M. Linden, Secretary of the Naples Zoological Station, according to Sr. Menard in *Cosmos* of December 17, 1898. He attaches elastic plates to the bow and stern of the boat, which act precisely as does the tail of a fish. They are bent by the pitching of the boat in a seaway, and the reaction of their forcible unbending, as well as that of their motion against the water while being bent, produces forward motion in the boat, in effect as the fish drives himself forward by springing its tail in lateral movements. Thus every motion of the boat on the surface of the waves produces greater or less acceleration.

The boat employed is stated to be four meters (13 feet) long, its driving plates 50 centimeters long (20 inches) and one-half that width. They are thicker at the point of support than toward their extremities, giving a proper flexure when pressed by the water into their impelling

curves. Other experiments have been made, also, at Berlin, which are thought to offer some encouragement, and it is suggested that such a plan may prove satisfactorily operative with large vessels.

The idea is, however, very old; no one knows where or when it originated. Some twenty-five years ago Mr. Gerner, a then well-known inventor and patent attorney, of New York, proposed a somewhat similar scheme, employing rafts or floats at the stern and on either side, which, with the rolling and pitching of the vessel, and the relative motion thus produced, should operate levers on board the vessel, and through them a system of mechanism which should drive a screw and thus impel the ship. Nothing came of it, however.

R. H. T.

REMEASUREMENT OF THE ARC OF PERU.

UNDER date of May 12th the Minister of Public Instruction and Fine Arts announced to the French Academy of Sciences the coming departure from Bordeaux, on the 26th of May, of M. Maurain, captain of engineers, and M. Lacombe, captain of artillery, for Quito. These two officers constitute a commission to visit the stations of the old arc of Peru, measured between 1736 and 1739 by Bouguer, La Condamine and Godin, with the view of a remeasurement of the arc and its extension so as to comprise from five to six degrees of latitude.

This action is hailed with pleasure by geodesists everywhere. It is the direct outcome of the renewal of the suggestion for its remeasurement made at the last meeting of the International Geodetic Association, at Stuttgart, in October, 1898.

The proposition that the work should be soon undertaken was brought up by the American delegate, Mr. E. D. Preston, of the U. S. Coast and Geodetic Survey, at that Conference, and his action was interpreted to mean that if France would not undertake it some other nation, probably ours, would take steps towards the remeasurement of the arc whose revision is considered of such great importance to geodesy.

LELAND STANFORD JR. UNIVERSITY.

BY the recent gifts of Mrs. Stanford, Leland Stanford Jr. University becomes the richest university in the world, far surpassing in its resources Harvard, Columbia or any foreign university. Situated where the development of civilization has been most rapid, and where its future promise is unlimited, under a wise and far-sighted administration, the University will become within a generation one of the greatest universities in the world. Correct details of the gifts and bequests of Senator Stanford, and of the gifts of Mrs. Stanford, will be of interest to readers of this JOURNAL.

The resources of the University consist of three great farms, aggregating 95,000 acres of land, deeded by Act of Legislature. On one of these farms, which constitutes the University Campus, buildings to the value of $1,000,000 were erected before Senator Stanford's death. By his will the University received $2,500,000 in cash, invested in interest-bearing bonds. During the litigation following his death Mrs. Stanford deeded to the University her own private fortune, amounting to about a million dollars. The bulk of his fortune was left by Senator Stanford by will to his wife, with the understanding between them that in case she survived him she would do all for the institution that he would have done. This wish she has carried out to the letter, although, as a matter of fact, idle litigation has prevented her from doing anything until very recently. By her recent gift she transferred the residue of the estate to the University, it being necessary to do this by deed of gift under the laws of the State. Mr. Stanford's purpose was a chivalrous one, emphasizing the equality of his wife in their mutual work. The property just turned over has a commercial value—judging from the revenue stamps put upon the deeds—of $35,000,000. It would probably bring in the market about $13,000,000. What its actual value may be only the future can determine. The income arising from this final gift is at present relatively small, as by agreement among the railroads, in bonds and stock of which it largely consists, the earnings are for a time to be used in freeing the property from debt and in making improvements.

SCIENTIFIC NOTES AND NEWS.

THE statue of Helmholtz, in the court of the University of Berlin, was unveiled on June 6th, in the presence of the German Emperor.

THE statue of Darwin by Mr. Hope Pinker, presented to Oxford University Museum by Professor Poulton, was unveiled on June 14th, with an address by Sir Joseph Hooker. The statue, which is of life-size and which somewhat dwarfs the figure of Newton, by the side of which it is placed, represents the philosopher in an attitude of meditation, his hands crossed on his breast.

DR. MILTON UPDEGRAFF, professor of astronomy in Missouri University, has been appointed, by President McKinley, professor of mathematics in the United States Naval Observatory.

THE Arago medal of the Paris Academy of Sciences was presented to Sir George Stokes on the occasion of his recent jubilee.

THE Council of the London Mathematical Society has awarded the sixth DeMorgan medal to Professor W. Burnside, F.R.S., for his researches in mathematics, particularly in the theory of groups of finite order.

MR. WILLIAM MARTINDALE was, on June 7th, elected President of the Pharmaceutical Society of Great Britain.

THE John Marshall prize for 1899 has been awarded to Jacob H. Hollander, Ph.D., associate professor of finance, for his publication entitled 'The Financial History of Baltimore.' The Marshall prize consists of a relief portrait in bronze of Chief Justice Marshall. It is awarded annually to a graduate of the Johns Hopkins University who has published the most important work in the department of history, politics and economics.

THE death, is announced, at the age of 74, of M. Nourrisson, professor of philosophy at the Lycée Napoléon since 1858, and since 1870 a member of the Academy of Political Sciences.

DR. THOMAS O. SUMMERS, professor of anatomy at the St. Louis College of Physicians and Surgeons, known for his researches on yellow fever, died by suicide on June 19th.

THE United States Civil Service Commission announces that it desires to establish an eligible register for the position of Scientific Aid, Department of Agriculture. Candidates are not required to appear at any place for examination, but should file statements with the Commission not later than August 1st. For the information of applicants the following statement is made, as received from the Secretary of Agriculture: (1) Applicants will be limited to graduates of colleges receiving the benefits of grants of land or money from the United States; (2) each applicant must file with the United States Civil Service Commission, Washington, D. C., a properly certified statement as to the length of time spent in college, the studies pursued, the standing in these studies, the special work it is desired to take up and the special qualifications for such work, and, finally, a thesis upon such special scientific subject as the applicant may select, or in lieu of this any literature on scientific subjects published over his own signature ; (3) the length of time any Scientific Aid may serve in the Department is limited to two years; (4) the salary shall not exceed forty dollars per month.

THE Maryland Geological Survey has started investigations in forestry in coöperation with the Division of Forestry at Washington, and Mr. George B. Sudworth, of the U. S. Department of Agriculture, has been detailed to work in Maryland and has already completed a forestry survey of Alleghany county. This work will be gradually extended throughout the State as fast as the topographic maps are completed. The Maryland Survey has also started biological investigations in Maryland under the supervision of Dr. C. Hart Merriam, of the U. S. Department of Agriculture, who has detailed members of his staff to begin a study of the distribution of the faunas and floras of the western section of the State. This work will be carried on as an adjunct to the Geological Survey of the State, and reports upon the life zones and areas of the State will be published from time to time by the State Geologist.

THE members of the Maryland Geological Survey recently made an extended trip along the shores of the Chesapeake Bay, upon one of

the State steamers, for the purpose of examining the stratigraphy of the Neocene and Pleistocene formations, which are to be the subject of special study during the present field season. Professor W. B. Clark, the State Geologist, was in charge of the expedition, and he had associated with him Messrs. H. F. Reid, E. B. Matthews and G. B. Shattuck as well as other members of the Survey. Dr. Arthur Hollick, of Columbia University, who is to undertake some investigations in paleobotany for the Survey, was one of the party. The expedition occupied ten days, and the trip extended into the lower Potomac basin as well as to several of the rivers of the Eastern Shore of Maryland.

WE have already called attention to the excursion arranged by the Union Pacific Railway Company to visit the fossil fields of Wyoming. Invitations have been sent to about 300 geologists and paleontologists, each of whom may bring at least one assistant with him. The party meets at Laramie on June 19th, and will be under the general direction of Professor Knight, of the University of Wyoming. The railway has issued a popular illustrated account of fossils in Wyoming, which can be obtained by application to one of their offices.

MR. A. J. BALFOUR, the government leader in the House of Commons, on June 27th assured a deputation representing the Royal Society and the Royal Geographical Society that the Chancellor of the Exchequer, Sir Michael Hicks-Beach, was prepared to give substantial aid to the proposed Antarctic expedition.

IT is reported in the daily papers that Dr. Nansen has resolved to enter the lists as an Antarctic explorer. Letters received in London from him state that he hopes to have an expedition organized and ready to start in 1902. He is at present engaged in preparing his plans, and will endeavor to shape them so that he may supplement the work which the British and German expeditions propose to accomplish. Dr. Nansen intends to go to Berlin for the International Congress of Geographers, and Sir Clements Markham and Sir John Murray will also be there to meet Professor von Drygalski, the leader of the German expedition. An Antarctic conference will be held, at which a general plan of action can be decided upon.

MR. H. J. MACKINDER, reader in geography at the University of Oxford, has just left England in charge of an expedition, the object of which is to make a thorough study of Mount Kenia, in British East Africa. The London *Times* states that the expedition is partly subsidized by the Royal Geographical Society, though a very considerable portion of the funds is contributed by Mr. Hausburg, one of the members of the expedition. Mr. Mackinder is also accompanied by two competent Swiss guides and two taxidermists and collectors. The expedition is well equipped with instruments, cameras and other means of carrying on scientific work. Dr. J. W. Gregory, when he visited Mount Kenia, succeeded in attaining a height of 17,000 ft., and his observations proved that further investigation would certainly yield interesting scientific results. Mr. Mackinder and his party propose to camp at a height of about 16,000 ft., and from this as a base they hope to make a good map of the whole mountain, ascend to its summit, journey all around it, investigate its glaciation and its geology, and make ample collections of animals and plants. As the expedition goes to work under specially favorable conditions, interesting results are expected. Mr. Mackinder hopes to spend at least a month on the mountain, and expects to be back in England about the beginning of October.

IT is intended that the first malaria expedition of the tropical medicine department of University College, Liverpool, should go to Sierra Leone in August. The expedition will be headed by Major Ross, and will include Dr. Sunnett, the demonstrator to the Liverpool school. The malarial season is at its height in August, and the conditions are then most favorable for research. Major Ross hopes to prove his theory that malaria is caused by the bites of a certain species of mosquito. The expedition will determine, by the methods which Major Ross employed in India, which are the malaria-bearing species in the locality chosen, and then inquire whether it is possible, by filling up the particular puddles in which they breed, to exterminate malaria in a given district.

IT has been decided that the Imperial School for the Study of Tropical Diseases, the establishment of which is due to the suggestion of Professor Koch, is to be settled at Hamburg. Professor Koch originally wished to have it in Berlin, but reasons of convenience have led to the substitution of Hamburg, where patients can be landed directly. The institution is for the present to be equipped to receive 30 patients.

MAJOR RONALD ROSS, I.M.S., inaugurated his first course of lectures on Tropical Medicine at University College, Liverpool, on June 12th, by an address on the 'Possibility of Extirpating Malaria,' in which he dealt in detail with the means of exterminating malaria-bearing mosquitos.

A REPRESENTATIVE of Reuter's Agency has had an interview with Doctor Henryk Arctowski, the Polish mineralogist and geologist of the *Belgica* Antarctic expedition. The *Belgica* expedition entered the Antarctic circle from the opposite direction to that in which the British expedition under Mr. Borchgrevinck is now working, Lieutenant Gerlache, with the *Belgica*, going *via* Cape Horn and the South Shetland Islands, while the British expedition started from Hobart for Victoria Land. Dr. Arctowski said that their first object was to make a voyage in the Antarctic, but beyond this there was on starting no definite program. It was intended to examine the various scientific conditions. On leaving Staten their object was to go direct to the south and to explore in the region of Grahamsland and Palmer Land, on which no landing had been made since their discovery, in the early part of the century. On February 13th, four weeks after leaving Staten Island, they left the newly-discovered land which they named Danco Land and in three days sighted Alexander I. Land. On the 28th the *Belgica* ran into the Antarctic ice pack. The temperature fell and the *Belgica* stuck fast. For a whole year she remained immovable, and for the first time human beings prepared to spend a winter in the Antarctic. They had quite expected to winter in the south polar region, but they had hoped to do so on land. For that purpose they had everything prepared, as it was their intention to build an observatory and depôt. They were, however, quite unable to find land on which to establish a depôt, and and had to remain on the ship. The Antarctic winter lasted two months, but owing to the bad weather that prevailed they did not see the sun for three months. They spent the winter in scientific work. All of them suffered a good deal during the Antarctic night, owing to defective circulation and heart trouble. All pulled through except Lieutenant Danco, who succumbed to heart failure in June of last year, and his remains were buried beneath the ice. The only other member of the expedition to lose his life was Carl Wiencke, a Norwegian sailor, who was lost overboard between Staten Island and the Antarctic. At the beginning of the present year they began cutting a channel through the ice for the *Belgica*. After much hard work they cut a passage 900 meters in length, the ship got free of ice on March 14th last. As soon as they got free of the ice they steamed direct for Cape Horn, and reached Punta Arenas, Patagonia, on February 27th. The scientific results were satisfactory and were quite what was expected. Unlike the Arctic the Antarctic has no animals. The only signs of life found on land were a number of very small insects, which were discovered among the penguin rookeries. In the water there was plenty of life. There were far more seals than in the north polar regions, a great quantity of small whales and an abundance of penguins. The Antarctic land they found to be entirely mountainous, absolutely glaciated—covered with snow and ice. In some places, where the cliffs were too precipitous for ice and snow to lodge, lichen and moss were found. Dr. F. A. Cook, of Brooklyn, surgeon and anthropologist of the expedition, has returned to New York and has given similar ac counts to the press.

UNIVERSITY AND EDUCATIONAL NEWS.

BY the will of the late R. C. Billings, of Boston, Harvard University, The Massachusetts Institute of Technology and the Boston Museum of Fine Arts each receive $100,000 and an additional $50,000 is given to the Massachusetts Institute of Technology for scholarships. The

will also contains a large number of bequests to hospitals and other charitable institutions.

At the commencement exercises of Brown University it was announced that $77,000 had been received in gifts, the names of the donors being in most cases withheld.

Dr. D. K. Pearson has given $125,000 to Olivet College.

At the annual commencement at Oberlin College it was announced that, in addition to the gift of $50,000 for a chemical laboratory, two other sums of $50,000 have been given, the names of donors being withheld.

In view of the bequest of $50,000 for the department of astronomy at Smith College by the will of Eliza Haven, won after long litigation, it has been decided that the department shall be known as the Elizabeth Haven School of Astronomy.

The following summary of students for the years 1898–'99 is taken from the catalogue just issued by the University of Minnesota:

Graduate students	195
Undergraduates; College of Science, Literature and the Arts	898
College Engineering and Mechanic Arts	151
The School of Mines	62
The School of Chemistry	9
Department of Agriculture	409
College of Law	447
Department of Medicine	475
Summer School for Teachers	380
	3,026
Counted more than once	101
Total	2,925
Total instructors	266
Students to each instructor	12

It has been decided to found a chair of pathological anatomy in the Laval University, Montreal. The list of subscribers to the fund which is being raised for the purpose is headed by the Archbishop of Montreal.

Dr. C. W. Super has been re-elected President of Ohio University. He occupied the position twelve years previous to 1896, when he declined re-election.

Dr. E. B. Matthews has been advanced to the position of associate professor of petrography and mineralogy, and Dr. G. B. Shattuck to the position of associate in physiographic geology, at Johns Hopkins University. In the Medical School Dr. L. F. Barker has been promoted to be associate professor of pathology, and Dr. R. G. Harrison to be associate professor of anatomy.

Mr. John L. Van Ornum, a graduate of the University of Wisconsin with the degree of B.S. in Civil Engineering, has been appointed professor of civil engineering in Washington University, where he has been for three years instructor. Mr. Van Ornum has lately been major of the Third U. S. V. Engineers.

Miss Florence M. Lyon, Ph.D. (Chicago), has been appointed assistant in botany, and Miss Annie I. Barrows assistant in zoology, at Smith College.

Of the twenty-two fellowships awarded in the Johns Hopkins University the following are in the sciences:

William Martin Blanchard, of Hartford, N. C., A.B., Randolph Macon College, 1894. Chemistry.

Charles Edward Caspart, of Baltimore, A.B., Johns Hopkins University, 1896. Chemistry.

Luther Pfahler Eisenhart, of York, Pa., A.B., Pennsylvania College, 1896. Mathematics.

Lawrence Edmonds Griffin, of Hamline, Minn., A.B. and Ph.B., Hamline University, 1895. Zoology.

Joseph Cawdell Herrick, of Virginia, A. B., University of Virginia, 1896. Physiology.

Charles A. Kraus, of Lawrence, Kan., S.B., University of Kansas, 1898. Physics.

Harry Taylor Marshall, of Baltimore, A.B., Johns Hopkins University, 1894, and M.D., 1898. Pathology.

John Charles Olsen, of Galesburg, Ill., A.B., Knox College, 1890. Chemistry.

Herbert Meredith Reese, of Baltimore, A.B., Johns Hopkins University, 1897. Physics.

George Burr Richardson, of New York City, S.B., Harvard University, 1895. Geology.

Richard Burton Rowe, of Clarksville, N. Y., Ph.B., Union College, 1896. Geology.

Dr. H. M. MacDonald, of Clare College, has been appointed University lecturer on mathematics at Cambridge University in the place of Professor Love.

At Oxford University Dr. Herbertson has been appointed lecturer in physical geography.

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